

Dr. W. Page May brought forward a communication by Prof. Elliot Smith and himself on the motor localisation in the lemur. He showed the area stimulation of which produced movements on the opposite side of the body, and demonstrated that the sequence of representation of movement was in agreement with that of Sherrington and Grünbaum on the ape. It also cleared up the discussion on the homologies of a small sulcus which had previously been described as postcentral and precentral, but which is really central, as Elliot Smith, arguing merely from morphology, pointed out two years ago.

He also described the results of localisation in the dog obtained by Elliot Smith and Wilson, who have shown that the excitable area is limited anteriorly by the crucial sulcus. This result was in harmony with the histological results of Cushing.

In a second communication Dr. Page May discussed the results of previous workers on the optic thalamus, and described some experiments he had made on this subject. He showed that following lesions in the thalamus certain motor disturbances were produced, and that descending paths could be traced from the thalamus into the anterior and lateral columns of the spinal cord. He also showed photographs and specimens of a descending tract hitherto undescribed in the posterior columns of the cervical and dorsal cord. This extended downwards from the thalamic region, and occupied a position near the middle line at the anterior end of Goll's column. In rare cases fibres could be traced into the lowest portions of the spinal cord.

Prof. G. S. Woodhead communicated the results of an investigation on joint-ill in the foal. This is an affection of especial importance to horse-breeders, in which, in addition to certain constitutional symptoms, marked stiffness and swelling makes its appearance in the joints, while at a later stage abscesses form. Investigation of the cause of this disease proved it to be due to a microorganism which gained admittance into the young animal through the cut end of the umbilical cord. From the practical point of view it was therefore evident that such precautions as are taken against septic infection in the case of the child at birth should also be taken in the case of the foal.

Dr. T. S. P. Strangeways gave an account of a committee of pathological research which is being founded with the object of investigating some of the more important diseases the pathology of which is as yet undetermined. The proposed lines on which the committee intends to act is to select some special disease and make an exhaustive study of it from all sides, a study which will last for two to three years. It is proposed to found a small hospital which shall be devoted entirely to cases of that disease during its period of study. The committee is to be a comprehensive one, and include all who will watch the course of the disease or who will undertake research work on the subject. These will report to a central body, which will also be responsible for the distribution of the collected facts and literature of the subject to those actively engaged in working upon it.

Prof. C. S. Sherrington and Miss S. C. M. Sowton communicated the results of an investigation into the amount of chloroform which, when administered to the heart, can dangerously embarrass its action. For this inquiry they had adopted the method, gradually evolved of recent years, of keeping the excised heart of a mammal alive by perfusing its blood-vessels with warm nutrient solutions. The heart used by them was that of the cat. The beating of auricle and ventricle was recorded graphically. The effect of chloroform was examined by allowing the perfusing fluid, pure saline, serum, or blood, as the case might be, to be replaced by a similar fluid to which chloroform in known quantity had been added. When this was done the chloroform showed its effect, practically at once, by diminishing the amplitude of the beat without altering its rate. The amount of the diminution was proportionate, within limits, to the concentration of the solution of chloroform. When exhibited in saline solution, chloroform showed a depressant action even in a dilution of 1 part in 150,000 of the saline solution. The full amount of the depression caused by a given solution was rapidly reached, e.g. in a minute, and then the continued administration of that solution caused no further depression—even if continued for half an hour at a time. That is to say, there is no cumulative action of

the drug detectable in the isolated heart so perfused for a period of half an hour. On the contrary, there was generally evidence of a slight waning of the depression as the exhibition of the drug was uninterruptedly maintained. This tolerance was, however, quite evanescent, for on interrupting the perfusion with the chloroform solution and then returning to it, the depression recurred in its original depth. On discontinuing the perfusion with chloroform solution and reverting to the chloroform-free fluid, the depression caused by the chloroform—unless the chloroform solution has been of great concentration—is extremely rapidly removed, even when the beat of the heart has been for many minutes practically abolished. This suggests the view that the effect of chloroform on the cardiac muscle is due to the formation of some easily dissociable compound between the chloroform and some active constituent of the tissue. It has been recently urged by Moore and Roaf that this constituent is a proteid, and in favour of this view is a further fact elicited in the present inquiry. On comparing the amount of depression of a chloroform solution of given concentration, in salt solution on the one hand and in blood on the other, it is found that the effect of that concentration in blood is much less than it is in salt solution. In other words, the effect of a chloroform solution of given concentration in blood is only equivalent to that of a solution barely one-twelfth as concentrated in salt solution. This can be explained by supposing that the salt solution, though it supports the beat of the heart, supports it less well than does blood; but the more important part of the explanation seems to be that the tension of the chloroform in the blood is much less than in the salt solution. In other words, the difference seems referable to some constituent of the blood taking up and holding, in a relatively inactive form, a considerable fraction of the chloroform added to it. The chloroform added distributes itself in that complex fluid according to a coefficient of partition. It is only what is left over freely dissolved in it which is available for acting on the heart tissue. Comparative estimations of the depressant effect in blood, serum, and saline solution show that serum is intermediate between the other two, so that evidently the corpuscles contain, in large measure, a substance that combines with the chloroform.

THE RELATION OF OXIDATION TO FUNCTIONAL ACTIVITY.¹

IN opening this discussion, Sir John Burdon-Sanderson said:—

In undertaking to open this discussion, I do not claim to contribute any results of my own researches or to speak on any subject *ex cathedra*, or with any degree of finality. I propose to state very shortly what seem to me the discussable questions, i.e. those respecting which we have experimental data, and to submit to the section those on which we need enlightenment.

The title is "Oxidation and Functional Activity." May I say that, without criticising it, I would ask for some latitude as regards the word oxidation. By oxidation is meant the formation of an oxide. Now we know that in the living organism oxygen may and does act without this happening, e.g. in those processes of which the oxygenating of the colouring matter of the blood is the type.

This is so important a distinction that I would suggest to substitute in these cases the term "oxygenation." The subject of our discussion would then be rightly stated as follows:—"The Relation between Oxygen and the Chemical Processes which Constitute Animal and Plant Life." The older notion of the part played by oxygen in the chemical processes of life was that it was a destroyer and not a maintainer of the chemical energies of the cell. We now recognise that oxygen may have a double function to perform, first as an element the presence of which is essential to the anabolic process by which living matter is built up, and secondly as equally essential to the disintegrative process which, taking muscular activity as the type of others, is associated with the performance of function. Of these two actions, in each of which oxygen is concerned—the constructive and the destructive—the second is better under-

¹ Report of a discussion before the Section of Physiology at the Cambridge meeting of the British Association, August 19.

stood than the first. It can be proved experimentally that in the living organism muscular work is accomplished by the transformation of a corresponding amount of chemical energy, however imperfectly we may understand how this transformation can occur at the temperature of the body. But as regards the participation of oxygen in the process of *restitution*, we are obliged to frame for ourselves a hypothesis and to clothe it in chemical language, according to which each elementary function is represented by a specific kind of living matter, *i.e.* by an aggregate of *living molecules* each of which is endowed in equal degree with the capacity of discharging the function which it represents. The difficulty lies in this, that the physiologist finds himself compelled to use chemical language in a sense which the chemist does not recognise. What we mean thereby is that the hypothetical living molecule consists of a permanent part which is not concerned in the performance of function, and of a collateral part which is used, *i.e.* disintegrated in every transition of the molecule from the inactive to the active state, to be immediately re-constituted when action ceases. This notion of *restitution* is the nutshell in which the difficulty lies. All that we know about it is that the access of oxygen is an essential condition for its accomplishment—oxygen not as an oxidiser, but as a restorer of functional capacity.

I now propose to pass from the general to the particular, *i.e.* to the consideration of the chemical process of life as it presents itself in particular organs, namely, first in muscle and in nerve centre, and secondly in such glands as have up to this time been investigated—two groups of structures representing what Bichat called respectively animal life and organic life. On the general principle that in all our investigations we should proceed from the known to the unknown, muscle must be taken first, for its metabolism is more within reach of investigation and is better understood than that of any other organ.

When oxygen enters the living substance of muscle it is not as an oxidiser, but as a preparer and builder up of material ready for explosion. For the muscle molecule receives two things from the blood, oxygen and oxidisable material, but these two do not combine as a mere result of juxtaposition or of encountering one another. As Ostwald says, "Der freie Sauerstoff ist ein sehr träger Stoff," at the temperature of the body. It cannot be brought into action in the living organism by a stimulus so long as it is in its free state. It must first become what Pflüger calls "intramolecular," and thereby change its *Trägheit* for mobility. The immediate effect of the access of oxygen is that the living substance of which it becomes a part becomes more susceptible to mechanical and electrical disturbance, *i.e.* more excitable than it was before. It requires, so to speak, to be wound up so as to become capable of discharging its oxidising function when awakened by a stimulus. Dr. Fletcher's experiments, to which I will return later, show that the more perfect this preliminary anabolic process is the more complete will be the catalysis.

You will, I think, agree with me that in different stages of the metabolic process which is associated with muscular function oxygen acts in different ways, at one time taking part in an integrating process for which we might, perhaps, employ the word oxygenation, at another in a process of oxidation, the molecule in which this occurs retaining its existence notwithstanding the disintegration of its oxidisable part.

We have now to pass on to the question how oxygen takes part in the functional activity of the central nervous system. The only part of that system which is within reach of experimental investigation is the spinal cord. We have to consider in how far the results of investigations in the cord and in muscle agree or differ.

Let me say on the threshold that our knowledge is largely due to work recently done at Jena and Göttingen under the direction of (or in cooperation with) Prof. Verworn. I must first ask your attention to the method.

More than thirty years ago Cohnheim taught us the use of a preparation which we used to call "the salt frog," in which the blood was replaced by physiological salt solution. He discovered that notwithstanding the defect of hæmoglobin, and consequently of oxygen, the chief functions of life could be carried on. With much more perfect methods Verworn has followed Cohnheim. The improvement consists

in this, that the circulation is maintained by mechanical means; so that by varying the rate of flow and the percentage of oxygen in the liquid the supply of oxygen to the cord can be increased or diminished at will. The effect produced is judged by the mechanical responses to reflex excitation, the indications given by which are rendered more delicate by the previous administration of a trace of strychnine. The first step in the experiment is to establish a normal state of things by the circulation of salt solution which has been freely exposed to air or oxygen. Under these conditions the response to stimulation of the surface consists of a succession of brief tetani, each lasting two or three hundredths of a second. The next step is to substitute salt solution which has been deprived of oxygen, and to observe that the reflex centre is gradually paralysed, as indicated by the fact that single tetani have taken the place of the serial responses, that on renewing the supply of oxygen the former state of things is restored, and, finally, that these changes may be repeated over and over again with the same result.

All these facts come under the general statement that while oxygen has no power of acting as a stimulus it increases the excitability of the centre, enabling it when excited to discharge itself so completely that after the discharge it is wholly incapable of responding. It further shows that oxygen shortens the time required for restitution to the normal—reintegration following disintegration—*anabolism* following *katabolism* so immediately that they may almost be considered as simultaneous.

If we compare the behaviour of oxygen in the centre with its behaviour in the muscle, we shall find that they differ chiefly in one particular, namely, in their time-relations. In both cases oxygen acts as a predisposing, not as an exciting cause of functional activity. In both cases a *tertium quid* is wanted—a liberating or letting off mechanism; but in the muscle the functional cycle is accomplished in scarcely more than 1/100 sec., whereas in the centre the effect occupies a few hundredths of a second, and the preparation for it a much longer period. There is therefore no difficulty in understanding why the so-called refractory period can be so easily observed and measured in the centre (while in the muscle its presence can only be inferred), a circumstance which is helpful as affording an additional evidence of the anabolic action of oxygen; for it is easy to show that the period in question is shortened by supply of oxygen, protracted by its absence.

We now come to the last point which I am anxious to submit to you—that of the relation of oxygen to the function of glands. I must begin by saying that it is in this part of our subject that the crux lies, for the investigation of the intimate metabolism of glands is beset with difficulties even greater than those of muscle and spinal cord.

Mr. Barcroft, to whose admirable researches we shall have occasion to refer repeatedly to-day, found as the result of his estimate of the oxygen and carbonic acid yielded by the blood circulating through the submaxillary gland under different conditions that this gland takes from the blood much more oxygen when excited by the chorda tympani than when at rest, no such effect occurring when the excitation had been rendered ineffectual by the previous administration of atropine. These observations gave good reason for believing that oxygen promotes the action of the cells, but afforded no evidence that this action is attended by a corresponding discharge of carbon dioxide. Similarly Prof. Starling, whose experiments were made with Mr. Barcroft's cooperation, found that when the pancreas is made to act by the injection of secretin a similar want of relation presented itself between the quantity of oxygen taken in and of carbon dioxide discharged. Finally, the comparison which has been recently made by Dr. Brodie (who will, I hope, explain to us his very admirable method) of the state of activity of the kidneys with the state of rest, points to the same conclusion as regards that organ. When diuresis was produced by the injection of urea, the clearest evidence was given of the increased demand for oxygen, the intake of which was very largely increased, but there was no indication that the ultimate products of oxidation found their way into the blood in quantities proportionate to the oxygen supplied.

Taking these data as our point of departure, what can we infer from them as regards the resemblances and differences between the two processes we have been considering.

viz. the functional activity of muscle and nerve centre on the one hand, that of gland on the other? The *obvious* contrasts which exist between secretion, muscular contraction, and reflex innervation need not be dwelt upon; the one thing with which we have to do is the nature of the chemical processes which are associated with these three forms of activity. If analogies are to be sought for it is here they will be found.

I submit to the section, and particularly to those members of it who are engaged in experimental researches on the subject, that the most important contrast between the concomitant chemical processes of gland function and muscle function consists in this, that whereas the former is not in any marked degree katabolic, the dominant process in the oxidation which is inseparably associated with the performance of muscular function *is* katabolic. We can readily account for this by reference to the fact that whereas the processes in muscle and in reflex centre are excitatory, those in glands are for the most part determined by stimuli of a very different kind from those that evoke nervous or muscular action, which last act exclusively as liberators of katabolic processes which are waiting to be discharged.

We have long been accustomed to regard the process by which, in muscle, chemical is translated into mechanical energy as explosive and instantaneous, and to take the end-result—the discharge of carbon dioxide—as the necessary concomitant of the production of heat and work; but as I remarked before, Dr. Fletcher recently published experiments which seem to show that for the attainment of this ultimate result it is essential that the muscle should be abundantly supplied with oxygen, in failure of which the oxidation process may stop short before its completion. I trust that we shall have the advantage of hearing to-day the further results of his researches, and particularly that he will give us information as to the relation between efficiency of contraction and the degree of completeness of the oxidation process.

In conclusion, the questions which present themselves are:—

(1) Whether it may be generally stated that the oxygen which is conveyed to the living matter of the tissues by the blood is *stored as "intramolecular oxygen" until it is required for the performance of katabolic functions*, and, if so, what is the *chemical relation* between the stored oxygen and the living molecules by which it is held? In submitting this question, I must again ask that the use of the term "*living molecule*" may be condoned.

(2) Whether it may be assumed that every *disintegrative process conditions a subsequent integrative process* by which the *status quo* is restored in the same living molecule; if so, does the anabolic effect which in muscle *follows* the change of form constitute as much a part of the response to stimulation as the catabolic effect which *precedes* the change of form? Can this be said of the chemical process which is associated with functional activity in gland?

Continuing the discussion, Dr. W. M. Fletcher pointed out that in the muscle cell only the katabolic processes had been effectively studied, and that these are characteristic of the special material giving energy for contraction—a material probably without analogue in the gland cell. The classical conceptions, due to Pflüger and to Hermann, of this material as a highly oxygenated substance breaking down, whether rapidly as in contraction or slowly as in survival periods, by inevitable stages to the ultimate stages of carbonic acid and water, irrespective of a contemporary supply of oxygen, were discussed and compared with the views of Verworn. It was urged that while a preliminary oxygenation of the living molecule may be admitted on wide grounds as the condition of irritability, such a conception by no means precludes the idea of additional oxidative processes occurring at some stage or stages of the katabolic disintegration. Disintegration effected under anaërobic conditions might, on this view, stop short of its normal end-products, these being replaced by representatives of earlier stages in the breakdown. Evidence in this direction has been got from three main classes of experiment. In the case of excised muscle, Dr. Fletcher's observations of the relation of oxygen supply to the yield of carbonic acid in rest and in activity, and to the onset of fatigue and of rigour, were described and held to be incompatible with the view that the entrance of oxygen conditioned the lability

of the molecule without further influence upon the subsequent course of katabolism. A second class of evidence was derived from the work of Chauveau and Kauffman, Ludwig and his pupils, Minot and others, upon the respiration of muscles with artificial circulation. An increased yield of carbonic acid due to activity was claimed or denied by these observers strikingly in proportion to the success with which the artificial circulation had been made to reproduce the normal. A third and large body of evidence is supplied by observers like Araki, Geppert, Meyer, and others, who have studied the results of muscular contraction with normal circulation, but under conditions of deficient oxidation. Anaërobic conditions always appear to diminish the amount of carbonic acid expired, while increasing the amount of acid products in the tissues, the blood, or the excreta.

Prof. N. Zuntz (Berlin), in a letter addressed to the section, referred to some of the points which it had been suggested should be dealt with in the discussion, in the following terms:—"Is one justified in drawing a hard and fast line between the anabolic and the katabolic processes on theoretical grounds? Would it not be more correct to take Pflüger's view and regard that process as the normal one in which every katabolically decomposed molecule is at the very moment of decomposition anabolically regenerated by taking up oxygen and oxidisable groups? In this case one would regard the katabolic processes, which render the molecular structure less stable and give rise to free affinities, as the factor which inaugurates and makes possible normal anabolism. In this connection, however, the fact remains that anabolism can also take place later on, if an element such as oxygen, necessary to the building up of the molecule, should be wanting at the time that the katabolic processes occur. It is accordingly a subject for investigation to decide whether subsequent regenerative processes occurring in the above manner take place as easily as normal assimilation occurring at the same time as the breaking down of the molecule, or whether they use up more energy if they occur later. I have already some data which tend to show that anabolism demands more energy if it has to take place at a period after the katabolic processes, but I dare not yet give any definite verdict on the question.

In regard to the question as to how far the metabolic processes may be the work of an oxidase, I should like to lay stress on the fact that the fundamental importance of innervation for katabolic processes in muscle is not easy to reconcile with the assumption that these processes are much affected by ferments. Neither does the great influence which the tension of a muscle has on oxidation processes in it harmonise with our knowledge of the action of enzymes.

Prof. T. G. Brodie described the results obtained in experiments, conducted in conjunction with Mr. Barcroft, upon the gaseous exchanges in the kidneys under the different conditions of rest and activity. In all cases they had found that the amount of oxygen taken in by a kidney which was made to secrete urine actively was greatly in excess of that absorbed by a resting kidney, while on the other hand the quantity of carbonic acid eliminated showed far slighter variations. In the greater number of their experiments they had found that the kidney at rest eliminated a greater volume of carbonic acid than it absorbed of oxygen. Their results thus indicated that the performance of work by the kidney was accompanied by an approximately proportional increase in the intake of oxygen, while the output of carbonic acid, although increased, was usually much less in amount. From the fact that the carbonic acid output was often in excess of the oxygen intake, it would seem that the final metabolic changes, as evidenced by the carbonic acid output, was a more gradual process, though the results they had obtained, up to the present, did not warrant the conclusion that the carbonaceous waste products resulting from the activity of the tissue were confined to carbonic acid only.

Mr. J. Barcroft, in discussing the metabolism of glands generally, pointed out that there were three methods which had been used for the investigation of their gaseous metabolism. In the first, an excised organ was kept in an enclosed space, and the surrounding air analysed. This method had been dealt with by Mr. Fletcher, who had pointed out that the method shed light on the katabolic

phase of activity only. In the second method the general gaseous exchanges of the body were watched during states of rest and activity of the organ to be investigated. This, however, was inapplicable to the glands of the body on account of their small size. The third method was that of measuring the blood gases, combined with an estimation of the rate of flow of blood through the gland.

Three glands have been studied by this method up to the present, the submaxillary, the pancreas, and the kidney. In the submaxillary gland the problem was very complicated, since the blood became concentrated, losing a tenth of its water or even more, and a considerable quantity of the carbonic acid left the gland in the secretion. After due allowance had been made for these disturbing factors, it appeared that the O intake and the CO₂ output were increased from three- to four-fold during stimulation of the chorda tympani nerve. As to how far these changes might be due to concomitant vascular changes was studied by examining the gaseous exchanges of an atropinised gland during stimulation of the chorda. It was found that this led to no increase in the amounts of O withdrawn, though an increased output of CO₂ was observed.

In the pancreas, which had been studied in conjunction with Prof. Starling, there was often no increased flow of blood synchronously with a secretion following an injection of secretin. They invariably found an increased absorption of O. Usually this increase was considerable; thus from eight comparisons the mean quantity of O taken up by the resting gland was 1.5 c.c. per minute, and by the active gland 5.5 c.c. per minute. These results were entirely in harmony with those brought forward by Prof. Brodie for the kidney.

It seemed, then, that glandular activity was accompanied by a large and instantaneous consumption of O, but that it was not necessarily accompanied by an increased CO₂ output.

Another point indicated was the magnitude of the gaseous metabolism of glands. In the submaxillary and in the pancreas, when at rest, about 0.025 c.c. to 0.035 c.c. of O per minute per gram of gland substance was absorbed. In the kidney Prof. Brodie had given one instance in which the organ was using as much as one-fifth of the total quantity of O taken in by the lungs, and it was common to find the O consumption of the kidney during diuresis to amount to one-tenth of the total taken by the whole body.

Prof. T. Clifford Allbutt suggested that the theories advanced as to the part played by oxygen offered some explanation of the fact, often experienced clinically, that the administration of oxygen gave relief to patients not only in cases where the heart and lungs were affected, but in many others also. He had long since given up the idea that oxygen was effective in these cases simply on account of the more favourable conditions under which the respiratory functions were placed. This was evidenced, for instance, by the tenacity with which the patients adhered to the treatment; for example, in cases of the vomiting of pregnancy, where its administration was often of great service.

Sir John Burdon-Sanderson, in bringing the discussion to a close, remarked that it had been an exceedingly fruitful one, and none the less so because the points under discussion had not been settled, but were still under investigation. It seemed clear to him that oxygen played two parts in metabolic processes, one of which was prominent in muscle, and was responsible for the final oxidation of explosive material, while the other, which was more accentuated in glands, was akin to a building up process, as it was involved in the elaboration of new material.

GEOLOGICAL NOTES.

THE puzzling and commonly fragmental remains styled by von Gümbel Lithiotis are the subject of an elaborate monograph by Dr. Otto Reis (*Abhandlungen der k.k. geol. Reichsanstalt*, Bd. xvii., Heft 6, 1903). After being considered as plants, from algæ to palms, for some twenty years, they settled down in 1890 as bivalve molluscs allied to oysters. Von Gümbel's revision, to this effect, is now revised by Dr. Reis, who points out that certain long ridges in the hinge-area represent teeth. Two genera, Cochlearites

and Lithiotis, are established, forming the Lithiotidae, a subfamily of the Spondyliidae. The minute structure of the shells is carefully described.

The interest aroused by the publication of "Bau und Bild Österreichs," recently reviewed in these columns, has called from Dr. Friedrich Katzer a series of papers (*Verhandlungen der k.k. geol. Reichsanstalt*, 1904, pp. 123, 150, 177, and 193), in which he hopes to fill some of the gaps still remaining in our knowledge of Bohemian geology. His work covers such widely diverse subjects as the zoning of the north-eastern coalfield under the Riesengebirge, and the magnetite-ores, occurring as separation-products in garnet-amphibolite, in the neighbourhood of Kutna Hora.

In the *Verhandlungen der k.k. geol. Reichsanstalt* for 1904 Herr R. Zuber (p. 200) explains his views as to the analogy of the Flysch deposits of Europe and those forming in tropical climates, with a heavy rainfall, in the neighbourhood of the mouths of rivers, whether these flow from continental land or from the members of an archipelago. Dr. H. Vetter (p. 134) interestingly connects the structure of the Little Karpathians with that of the eastern Alps on the one hand, and of the true Karpathians on the other. The Flysch of the north side of the Alps thus reappears from under the Vienna basin, and passes into the Karpathian Sandstone series; while the characteristic Karpathian "Klippen" are also traceable in this connecting range. The author regards the so-called Silurian grauacke of Hainburg, down against the Danube, and similar rocks of the Leitha range, as in reality the equivalents of the Liassic beds in the Little Karpathians.

The *Jahrbuch der k.k. geol. Reichsanstalt* of Vienna usually contains more massive papers than the *Verhandlungen*, though it is difficult to discriminate between the two in scientific value. In vol. liii. of the former (1903), pp. 169-252, Dr. O. Ampferer describes, with numerous sections, the Triassic and Jurassic mountains that form the impressive broken country of crag and forest between Innsbruck and the Achensee. He illustrates his view of their structure (plate x.) by a skilful drawing of a relief-model, much in the American manner, thus emphasising the simpler anticlinal and synclinal structure in the south, and the great overfold of Trias upon a recumbent Jurassic synclinal in the Gamsjoch area. It seems possible, as the author points out, that the whole highland of Triassic rocks rests upon an underfolded and underthrust knot of younger strata. Dr. Ampferer modestly regards his own researches as supplementing, and correcting at certain points, those of Prof. Rothpletz and his Bavarian colleagues. He adds, moreover, details as to the glacial phenomena throughout the district. Herr E. Fugger's paper in the same volume (p. 295) describes the foothills of the Alps in the famous Salzkammergut area, where the Flysch, with its dubious fossils, forms the oldest series, and is confidently ascribed to the Upper Cretaceous epoch. Dr. F. Ryba (p. 351) revises and amplifies the list of fossil plants from the Cannel Coal of Nýran in Bohemia, and Dr. Waagen (p. 443) adds to our knowledge of the small brachiopods characteristic of the Tyrolean Trias.

We have received the August number of the *Quarterly Journal* of the Geological Society, which contains a well illustrated article on the history of volcanic action in the Phlegrean Fields by Prof. Giuseppe de Lorenzo; an account of the discovery of a human skeleton in Gough's Cavern, at Cheddar, by Mr. H. N. Davies, who regards the remains as of late Palæolithic age, although in the discussion which followed the reading of the paper this antiquity was questioned; and among other papers there is an important one on the age of the Llyn-Padarn dykes in Carnarvonshire by Mr. J. Vincent Elsdon, who regards these deep-seated intrusions as having taken place during the latest stage of the Bala eruptions.

The summary report of the Geological Survey Department of Canada for 1903 has been issued by Dr. Robert Bell, the acting deputy head and director. As usual, the energies of the staff have been given mainly to investigating and aiding the development of the mineral resources of the country. Field work has been carried on in Yukon territory, in British Columbia, in the Keewatin district, in Ontario, Quebec, New Brunswick, and Nova Scotia. In connection with the large output of coal which is now going on in